

feared, will be called in to explain many of the illustrations.

(3) The laboratory notes on water analysis are intended for the use of engineers, who, it is presumed, have already received a sound training in practical and theoretical chemistry. For there are no equations or explanations of the reactions involved in the various processes, which are described in the briefest manner, so briefly, indeed, that we should doubt if some of the operations could be successfully carried out. Thus, the reader is told (p. 19) to "neutralise with 1 c.c. of the reagent and compare the standards," without other reference.

It seems unnecessary and merely confusing to introduce indiscriminately both centigrade and Fahrenheit scales, and an over-elaboration to count the drops of a reagent the strength of which is not given (p. 20).

It may also be pointed out that the method described as Dr. Thresh's (p. 21) is usually known as Forchhammer's or Tidy's process.

J. B. C.

OUR BOOK SHELF.

Mental Pathology in its Relation to Normal Psychology. A Course of Lectures delivered in the University of Leipzig. By Dr. Gustav Störing. Translated by Thomas Loveday. Pp. x+298. (London: Swan Sonnenschein and Co., Ltd., 1907.) Price 10s. 6d.

THE bearing of the study of abnormal mental processes upon general psychological doctrine has long been understood. In some cases invaluable light may be thrown upon the normal nature of a complicated psychosis by the abnormal heightening or lowering in degree of one of its constituents; in other cases a pathological phenomenon may supply the "negative instance" that checks the harmful progress of a plausible but erroneous theory. Thus the leading pathological cases are familiar to English readers from their appearance in one or other of these capacities in the pages of several treatises on general psychology.

Nevertheless, Prof. Loveday is undoubtedly right in thinking that a systematic collection of such cases by a psychologist competent to select them judiciously, to describe them accurately but without unnecessary clinical detail, and to illuminate them by a cautious commentary, would be a useful addition to the student's library. Further, we believe him to be right in thinking that Dr. Störing's lectures prove that he possesses these qualifications in at least as high a degree as any other writer on the subject.

The besetting sin of the morbid psychologist is to erect elaborate and novel systems of interpretation upon a too narrow basis of fact. Dr. Störing avoids this fault, and exhibits a conservatism and restraint which will favourably impress even those who, like his translator, do not find themselves able to accept all his conclusions.

No one who is acquainted with the present unsettled state of psychological opinion upon fundamentals will be surprised to find himself frequently unable to agree with the author's view, or at least compelled to translate his interpretations into what he deems a more satisfactory psychological idiom. But in any case it remains true that on fundamental questions of psychological theory—such as the nature of perception and of the consciousness of self—and on questions of great importance in the practical science of pedagogy—such as the teaching of reading and writing, and the "training of the will"—Dr. Störing's cases (though

they need supplementing and correction by more modern instances) throw a light the strength of which is due largely to the way in which the several rays have been disposed and concentrated.

It is doubtful whether the translator did well to decline the task of finding English equivalents for such Teutonisms as "disease-picture," which occurs rather frequently in his pages. In a second edition he should certainly Anglicise the index letters of his diagrams, which are, as they stand, provokingly difficult to use.

The Evolution of the Atmosphere as a Proof of Design in Creation. By John Phin. Pp. 191. (New York: The Industrial Publication Company, 1908.)

ACCORDING to its subtitle, this work is "a simple and rigorously scientific reply to modern materialistic atheism," and, after perusing it, we find no reason to dispute the first portion of the description. But when we see "rigorously scientific," we feel inclined to question the accuracy of the descriptive phrase.

The purpose and tenor of the volume may be gathered from the following extract (p. 184):—"Any one who will carefully read the works of Haeckel, Tyndall, Huxley and men of that stamp cannot fail to see that their intense hatred of ecclesiasticism has swayed their logic, embittered their language and even led them to distort their facts when they came to write about anything relating to the religious faith taught in the churches."

The greater part of the book is taken up by definitions, and by the demonstration of simple scientific experiments illustrating the physical and chemical properties of the atmosphere, the idea being to show that, had not an intelligent creator adjusted the proportions of terrestrial elements to the very finest conceivable degree, the atmosphere could not have been suitable for man's existence. That such creative design must have superintended the composition of the primitive nebula of the solar system, at least, and also its proper partition, is not stated by Mr. Phin, although to be "rigorously scientific" this aspect would, presumably, have to be considered.

The probable sequence of the evolution of the atmosphere is reasonably stated on lines similar to those indicated in Lockyer's "Inorganic Evolution." But the "proof" of design apparently consists of Mr. Phin's statement that, because man exists, therefore an intelligent designer mixed the eighty or so terrestrial—speaking more logically "cosmical"—elements in such proportions that, after all their combinations and dissociations, their expansions and condensations, there remained just enough nitrogen, oxygen, &c., uncombined, to provide an atmosphere exactly suited to the requirements of the preconceived organic life.

That such life might have developed with, say, even a little less oxygen, or even a little of the uncondensed sulphuric acid he mentions, and yet not have been radically different in form, is not considered by Mr. Phin; yet we know that one species, of one age and of one development, is able to exist under very different conditions of atmospheric pressure and composition.

The author concedes, for the moment, that previous "evidences" have been materially weakened by the theory of organic evolution, and gives that as his reason for considering "inorganic" phenomena, wherein Haeckel's "sexual cell-love" is, presumably, inoperative.

The readers to whom the book will appeal will no doubt feel reassured by the author's statement that, whilst betting or gambling for gain is immoral, "the throwing of dice . . . or the tossing of coins for the purpose of determining the scientific principles involved in the theory of probability" is innocuous.

W. E. ROLSTON.

Essays and Addresses. By the late J. H. Bridges. With an introduction by Frederic Harrison. Pp. xxi + 307. (London: Chapman and Hall, Ltd., 1907.) Price 12s. 6d. net.

The essays included in this volume (unobtrusively edited by Prof. L. T. Hobhouse) form an admirable memorial of one of the noblest spirits that have been touched to fine issues by the "religion of humanity." It is, naturally enough, chiefly as a splendid evangelist of the Positivist movement that Dr. Bridges is considered in the introduction—itsself an interesting and illuminative essay—which Mr. Frederic Harrison has contributed to the book. But there is no reader, however unsympathetic with the Communist propaganda, who could rise from the perusal of these essays without having acquired deep admiration for the earnestness and spiritual charm, the learning, worn lightly as a flower, and presented with extraordinary vividness and freshness, the wonderful industry, fecundity, and versatility of the man whose literary achievements were the fruit of the leisure hours of a busy physician and hard-worked Government inspector.

The scientific reader who first made Dr. Bridges' acquaintance as the learned and indefatigable editor of Roger Bacon will accept almost as a matter of course the masterly summary of his long study of the great Franciscan, delivered as a university extension lecture in 1903. He will find in the oration on "Harvey and his Successors" merely another delightful example of the combination of critical, historical, and expository powers that illuminated so effectively the "Opus Majus." He will be prepared also for the familiar knowledge of the mediæval world shown in the two essays on Dante. But in these latter essays, particularly in the one entitled "Love the Principle," he will have revelation of spiritual powers perhaps unsuspected and of the noblest type. Moreover, his progress through the book will constantly deepen the impression that, even more admirable than the ability, the industry, and the taste that made Dr. Bridges so interesting and instructive a critic of topics ranging from Thales to Calderon and Diderot, was the self-sacrificing enthusiasm ever burning at the core of his indefatigable life.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Upper Air Temperatures.

WITH reference to Dr. Chree's letter published in NATURE of April 1, may I state that the conditions I postulated were violated in every one of the cases quoted in his former letter? The ascents were all made *after* sunrise and *before* sunset. They cannot, therefore, be fairly used as evidence to suggest that my conclusions were inaccurate in the direction of underestimating the influence of radiation. At the same time, it may be worth while to consider these cases.

Dr. Chree specified three occasions on which the differences between the temperatures recorded by two instruments of different types exceeded 2° C., the maximum differences being $2^{\circ} \cdot 1$, $2^{\circ} \cdot 5$, $2^{\circ} \cdot 9$ C. The first, as he stated, was probably due to a scale or zero error, one thermometer being continually below the other. The third, on which he lays most stress, occurred in an ascent at Uccle on February 7, 1907. The readings of both instruments agreed during the ascent, the greatest difference being $0^{\circ} \cdot 8$ C. (or $1^{\circ} \cdot 1$ C. at the highest point). During the descent larger differences occurred. Now, at the time of observation, the sky was covered by a veil of cirro-stratus, and it appears extremely probable that the

instruments, in descending from cold to warmer, perhaps saturated, air, would be affected by condensation of ice-vapour. The difference in exposure and type, combined with the bad conductivity of hoar-frost, may quite reasonably account for the differences between the temperatures of the two instruments which arose when they left the isothermal region. In addition to this, at the time of maximum difference the downward velocity was about 10 m.p.s., and there would be some lag in the instruments. This descent was, in fact, exceptional.

In the second case, an ascent at Strassburg on the same day, the readings indicate a slight lag in one instrument until the lowest temperature is reached. The sudden passage to a relatively warm upper layer was accompanied by a sudden jump of $1^{\circ} \cdot 2$ C. in the difference between the readings of the two instruments. The type of instrument which shows the lower temperature is the same as that which showed the higher temperature in the Uccle descent. This is just what we should expect if the instruments passed from a saturated layer, in which they became covered with hoar-frost, to a drier region. There is no record of the upper clouds at Strassburg at the time of the ascent, but it occurred simultaneously with the Uccle ascent, so that the explanation is a possible one.

In an earlier letter Dr. Chree suggested the possibility of errors of $\pm 10^{\circ}$ F. in the instrumental records. In order to show as fairly and clearly as possible the errors that may arise, I have taken, for Munich, all the cases from January, 1907, to March, 1908, in which the readings from two types of instrument were obtained, and the following table gives the height of the ascent, the extreme differences that occurred, and the mean of the absolute values of the differences at all the points for which they are published. The types of instrument were the same as those considered by Dr. Chree.

Height in kilometres	Extreme values of $T_1 - T_2$ (degrees C.)			Mean value of $ T_1 - T_2 $
10.5 ...	$1^{\circ} \cdot 2$...	$-1^{\circ} \cdot 6$	0.7
10.5 ...	0.5 ...	$-1^{\circ} \cdot 5$	0.5
9.8 ...	0.6 ...	$-1^{\circ} \cdot 3$	0.5
14.8 ...	0.6 ...	$-1^{\circ} \cdot 6$	0.3
11.0 ...	0.2 ...	$-0^{\circ} \cdot 6$	0.2
12.4 ...	$2^{\circ} \cdot 4$...	$-0^{\circ} \cdot 2$	0.5
13.5 ...	0.5 ...	$-0^{\circ} \cdot 2$	0.2
12.7 ...	$1^{\circ} \cdot 8$...	$-0^{\circ} \cdot 7$	0.5
13.0 ...	$1^{\circ} \cdot 0$...	$-0^{\circ} \cdot 2$	0.3
17.0 ...	$3^{\circ} \cdot 5$...	$-0^{\circ} \cdot 9$	$1^{\circ} \cdot 1$
13.0 ...	$3^{\circ} \cdot 0$...	$-0^{\circ} \cdot 6$	0.9
12.9 ...	$1^{\circ} \cdot 3$...	$-1^{\circ} \cdot 2$	0.5
14.2 ...	$1^{\circ} \cdot 1$...	$-1^{\circ} \cdot 1$	0.5
13.4 ...	$1^{\circ} \cdot 5$...	$-1^{\circ} \cdot 3$	0.5
14.8 ...	$1^{\circ} \cdot 4$...	$-2^{\circ} \cdot 8$	0.8
16.0 ...	$1^{\circ} \cdot 7$...	$-2^{\circ} \cdot 9$	0.8

In interpreting these results, it ought to be borne in mind that they are chiefly from *ascents*, and include errors owing to lag, which could be largely eliminated in dealing with the observations. The records I have seen usually show that the thermometer, which is higher in the ascent, is lower during the descent, and that the lag occurs almost entirely in the worse instrument, so that the differences are representative of the absolute errors arising from this cause. Considering the very many sources of error to be guarded against, especially the difficulty of testing the instruments at very low temperatures under the conditions to which they are to be exposed, I can only regard these results as a tribute to the care and ingenuity displayed by those engaged in the experimental exploration of the upper air.

Dr. Chree does undoubted service in directing attention to the need for great care in testing and comparing instruments, but I think he is inclined to be a little unjust to those who are tackling the difficulties of upper-air investigation and nomenclature. These difficulties are exemplified by the examples he quoted and by a term which he himself accepts, apparently without demur, when he describes a phenomenon as an "inversion of temperature."

Personally, I am quite prepared to discard the term "isothermal" when another is suggested which is short, equally expressive, more accurate, and more characteristic. The greatest variation of temperature in a vertical direc-